PROCEEDINGS
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THE ROYAL SOCIETY.

1837—1838. No. 31.

December 7, 1837.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

No paper was read.

December 14, 1837.

JOHN GEORGE CHILDREN, Esq., Vice-President, in the Chair.

The reading of a paper, entitled "On low Fogs and stationary Clouds." By William Kelly, M.D. Communicated by Captain Beaufort, R.N., F.R.S., &c., was resumed and concluded.

The object of the present paper is to point out the circumstances which influence the formation of low fogs, and to show what analogy exists between the causes that produce them and those that occasion certain forms of clouds, which may be considered as differing from fogs only in position. Having been attached for several years to the naval party employed in the survey of the gulf and river of St. Lawrence, the author had ample opportunities of observing the phenomena in question. He concludes that the fogs described occur chiefly when the air is nearly saturated with moisture, and when at the same time the temperature of the water on which they rest either exceeds that of the air, or is considerably below it. These fogs are generally very dense, often limiting the sphere of vision to a few fathoms; but seldom extend to any considerable height. They do not often cover the land to any distance from the shore; and the tops of the hills, close to the water's edge, are clear, while the bases, or sides, are enveloped in the mist.

The following papers were then read:—

"On the Colours of Mixed Plates." By Sir David Brewster, K.G.H., F.R.S., &c.

In the prosecution of his optical inquiries, the author was induced to study the phenomena of mixed plates, (originally discovered by Dr. Young, and described by him in the Philosophical Transactions for 1802,) as he had observed similar appearances in various mineral bodies under analogous circumstances, to which he had been led to ascribe an origin different from that assigned by Dr. Young. In order to obtain a more distinct view of these colours, Sir David Brewster employed, instead of the substances used by Dr. Young, the white of an egg, beat up into froth, and pressed into a thin film between plates of glass. From observations of the colours exhibited by plates so prepared, and also by the edge of a thin film of nacrite in contact with copaivi balsam, the author deduces the conclusion, that all these phenomena, as well as those often seen in certain specimens of mica through which titanium is disseminated, and also in sulphate of lime, are cases of diffraction, where the light is obstructed by the edges of very thin transparent plates placed in a medium of different refractive power. If the plate were opake, the fringes produced would be of the same kind as those often noticed, and which are explained on the principle of interference; but, owing to the transparency of the plate, fringes are produced within its shadow; and, owing to the thinness of the plate, the light transmitted through it is retarded, and, interfering with the partial waves which pass through the plate, and with those which pass beyond the diffracting edge with undiminished velocity, modify the usual system of fringes in the manner described by the author in the present paper.

"Of such Ellipsoids, consisting of homogeneous Matter, as are capable of having the Resultant of the Attraction of the Mass upon a Particle in the Surface, and a Centrifugal Force caused by revolving about one of the Axes, made perpendicular to the Surface." By James Ivory, K.H., M.A., F.R.S. L. and Ed., Inst. Reg. Sc., Paris, Corresp. et Reg. Sc. Gotting. Corresp.

Lagrange, who has considered the problem of the attractions of homogeneous ellipsoids in all its generality, and has given the true equations from which its solution must be derived, inferred from them that a homogeneous planet cannot be in equilibrium unless it has a figure of revolution. But M. Jacobi has proved that an equilibrium is possible in some ellipsoids of which the three axes are unequal and have a certain relation to one another. His transcendental equations, however, although adapted to numerical computation on particular suppositions, still leave the most interesting points of the problem unexplored.

The author of the present paper points out the following property as being characteristic of all spheroids with which an equilibrium is possible on the supposition of a centrifugal force. From any point in the surface of the ellipsoid draw a perpendicular to the least axis, and likewise a line at right angles to the surface: if the plane passing through these two lines contain the resultant of the attractions of all the particles of the spheroid upon the point in the surface, the equilibrium will be possible, otherwise it will not. For the resultant of the centrifugal force and the attraction of the mass must be a force perpendicular to the surface of the ellipsoid, which requires that the directions of the three forces shall be contained in one plane. This determination obviously comprehends all spheroids of revolution; but, on account of the complicated nature of the attractive force, it is difficult to deduce from it whether an equilibrium be possible or not in spheroids of three unequal axes, a problem which is unconnected with the physical conditions of equilibrium, and which is a purely geometrical question respecting a property of certain ellipsoids.

The author then enters into an analytical investigation, from which he deduces the fundamental equation

$$\mathrm{B} - \frac{\mathrm{A}}{1+\lambda^2} = \mathrm{C} - \frac{\mathrm{A}}{1+\lambda'^2} \quad \ldots\ldots\ldots \quad (1.)$$

the three axes of the ellipsoid being

$$k, \quad k\sqrt{(1+\lambda)}, \quad k\sqrt{(1+\lambda'^2)},$$

and A, B, C, constants, afterwards expressed by certain definite integrals. He then remarks that every ellipsoid which verifies this formula is capable of an equilibrium when it is made to revolve with a proper angular velocity about the least axis; for, in this case, the centrifugal force will be represented in quantity and direction by a line such that the resultant of this force and the whole attraction of the ellipsoid upon a point in the surface will be perpendicular to the surface. Lagrange had concluded that the equation (1), which results immediately from his investigations, admits of solution only in spheroids of revolution, that is when $\lambda = \lambda'$ and $\mathrm{B} = \mathrm{C}$; but by expressing the functions A, B, C in elliptic integrals, M. Jacobi has found that the equation may be solved when the three axes have a particular relation to one another. In order to ascertain the precise limits within which this extension of the problem is possible, and to determine the ellipsoid when the centrifugal force is given, the author has recourse to the equations of Lagrange, which contain all the necessary conditions, and he deduces the equations

$$f = \mathrm{B} - \frac{\mathrm{A}}{1+\lambda^2}, \quad f = \mathrm{C} - \frac{\mathrm{A}}{1+\lambda'^2}, \quad \ldots \quad (2.)$$

where f represents the intensity of the centrifugal force at the distance equal to unity from the axis of rotation, and remarks that these equations coincide with the equations of Lagrange. Substituting for A, B, C certain definite integrals given in the *Mécanique Celeste*, he deduces three equations expressing the value of g, the ratio of the intensity of the centrifugal to that of the attractive force, one of these being expressed in terms of the density and the other two in the form of definite integrals; and then remarks that "these equations comprehend all ellipsoids that are susceptible of equilibrium on the supposition of a centrifugal force."

He then applies these equations to the more simple case of the spheroid of revolution, where $\lambda = \lambda' = l$, and determines the value of l

$$l = 2{\cdot}5293,$$

and the corresponding maximum value of $g = 0{\cdot}3370$, and remarks that, " with respect to spheroids of revolution, it thus appears that an equilibrium is impossible when g, or its value in terms of the density, is greater than 0·3370. In the extreme case, when g is equal to 0·3370, there is only one form of equilibrium, the axes of the spheroid being

$$k \text{ and } k \sqrt{\{1 + (2{\cdot}5293)^2\}} \text{ or } 2{\cdot}7197\, k;$$

but when g is less than 0·3370 there are two different forms of equilibrium, the equatorial radius of the one being less, and of the other greater than 2·7197 k, k being the semi-axis of rotation.

The number of the forms of equilibrium in spheroids of revolution, he remarks, is purely a mathematical deduction from the expression of the ratio of the centrifugal to the attractive forces; and as this has been known since the time of Maclauriu, the discussion of it was all that was wanted for perfecting this part of the theory.

Returning to the general equations of the problem, the author deduces the equations

$$g = \frac{d\,\varphi}{d\,p}\,p$$

$$o = \frac{d\,\varphi}{\tau\, d\,\tau},$$

where φ is a definite integral, such that

$$g = \frac{d\,\varphi}{d\,\lambda}\,.\,\lambda, \quad g = \frac{d\,\varphi'}{d\,\lambda'}.$$

$$p = \lambda\,\lambda' \text{ and } \tau^2 = (\lambda - \lambda')^2,$$

which equations apply exclusively to ellipsoids with three unequal axes, and solve the problem with regard to that class. From these he derives another equation, which he states is no other than a transformation of his first fundamental equation, and is equivalent to other transformations of the same equation found by M. Jacobi and M. Liouville.

He also remarks that a limitation of one of the constants, which the verification of this formula requires, agrees with the limitation of M. Jacobi; and further, that the relations which may subsist between the constants proves that there does exist an infinite number of ellipsoids not of revolution, which are susceptible of an equilibrium.

After determining the corresponding limits of these relations of the constants, p being contained between the limits 1·9414 and 1, while τ^2 increases from zero to infinity, he remarks that an elliptical spheroid formed of a homogeneous fluid can be in equilibrium by the action of a centrifugal force only when it revolves about the least axis.

He next deduces the general value of g (the ratio of the forces), and thence its value in one extreme case, when $\tau^2 = 0$, or when

λ and λ' are equal, and remarks that this is no other than the determination of g in a spheroid of revolution having its axes equal to

$$k \text{ and } k\sqrt{2\cdot9414} = k \times 1\cdot7150.$$

In the other extreme case, when τ^2 is infinitely great, g is zero,

From this investigation the conclusion is arrived at, that for every given value of τ^2 there is only one value of p, and only one ellipsoid; and that to every such ellipsoid there is an appropriate value of g: and, further, that for every possible value of g there will be only one value of τ^2, and consequently only one ellipsoid susceptible of an equilibrium.

Also the reading of a paper, entitled, "Experimental Researches in Electricity." Eleventh series. By M. Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, was commenced.

December 21, 1837.

FRANCIS BAILY, Esq., Vice-President and Treasurer,
in the Chair.

The reading of Mr. Faraday's eleventh series of Experimental Researches in Electricity was resumed, but not concluded.

The Society then adjourned over the Christmas vacation to meet again on the 11th of January next.

January 11, 1838.

JOHN GEORGE CHILDREN, Esq., Vice-President, in the Chair.

The ballot for Bryan Donkin, Esq., was postponed in consequence of the number of Fellows required by the Statutes not being present.

The reading of a paper, entitled "Experimental Researches in Electricity," Eleventh Series, by Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, &c., was resumed and concluded.

The object of this paper is to establish two general principles relating to the theory of electricity, which appear to be of great importance; first, that induction is in all cases the result of the actions of contiguous particles; and secondly, that different insulators have different inductive capacities.

The class of phænomena usually arranged under the head of *induction* are reducible to a general fact, the existence of which we may recognise in all electrical phænomena whatsoever; and they involve the operation of a principle having all the characters of a first, essential and fundamental law. The discovery which he had already made of the law by which electrolytes refuse to yield their elements to a current when in the solid state, though they give them forth freely when liquid, suggested to the author the extension of analogous explanations with regard to inductive action, and the possible reduction of many dissimilar phænomena to one single comprehensive law. As the whole effect upon the electrolyte appeared to be an action of the particles when thrown into a peculiar polarized state, he was led to suspect that common induction itself is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term *induction*, never occurs except through the intermediate agency of intervening matter. He considered that a test of the correctness of his views might be obtained by tracing the course of inductive action; for if it were found to be exerted in curved lines it would naturally indicate the action of contiguous particles, and would scarcely be compatible with action at a distance. Moreover, if induction be an action of contiguous particles, and likewise the first step in electrolyzation, there seemed reason to expect some particular relation of this action to the different kinds of matter through which it is exerted; that is, something equivalent to a specific electric induction for different bodies; and the existence of such specific powers would be an irrefragable proof of the dependence of induction on the intervening particles. The failure of all attempts to produce an absolute charge of electricity of one species alone, independent of the other, first suggested to the author the notion that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.

The author describes various experiments, with the view of showing that no case ever occurs in which an absolute charge of one species of electricity can be given. His first experiments were conducted on a very large scale: an insulated cube, twelve feet in the side, consisting of a wooden frame, with wire net-work, every part of which was brought into good metallic contact by bands of tin foil, had a glass tube, containing a wire in connexion with a large electrical machine, passed through its side, so that about four feet of the tube entered within the cube and two feet remained without; but it was found impossible in any way to charge the air within this apparatus with the least portion of either electricity.

For investigating the question whether induction is an action of contiguous particles, the author employed, as an electrometer, the torsion balance of Coulomb with certain alterations and additions; and for deciding that of specific inductive capacity, a new apparatus, constructed for that express purpose. This apparatus consisted of two hollow brass spheres, of very unequal diameters, the smaller placed within the larger, and concentric with it; the interval between the two being the space through which the induction was to be effected. The apparatus had a tube in the lower part, furnished with a stop-cock, by means of which it might be connected with an air-pump or filled with any required gas. In place of the lower hemispherical shell of air, occupying the interval between the two spheres, any solid dielectric, of the same form, such as shell-lac, glass, or sulphur, might be substituted. Two of these instruments, precisely similar in every respect, were constructed, and the author ascertained that the inductive power was the same in both, by alternately charging each and dividing the charge with the other, and finding that, in all cases, the charge remaining in the one, and also that received by the other, was very nearly half the original charge.

The experiments on which the author principally relies in support of the correctness of his views relative to induction being exerted in curved lines, are the following: a brass ball being laid on the top of an excited cylinder of shell-lac placed vertically, the charge which a carrier ball received when brought to different points near to the brass sphere was measured by means of the electrometer; and it was inferred, from the character of the electricity, that the charge was one by induction, and from its measure, that it proceeded in curved lines. By substituting for the brass sphere a disc of metal above the shell-lac cylinder, it was found that when the carrier ball was brought near to the middle of the disc no charge was communicated, although a sensible one was obtained at the edge of the disc, and also at a point above its centre, farther removed from the excited cylinder. Corresponding and very striking results were obtained when a brass hemisphere was placed on the top of the cylinder of lac. The charge communicated at the centre of the hemisphere was only one-third of that obtained at the edge of its periphery; but by taking it at a point at some height above the centre, and consequently much farther removed from the inducing cause, the charge was nearly equal to that of the periphery. Here, the author remarks, the induction fairly turned a corner, exhibiting both the curved lines or courses of its action, when disturbed from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a lateral tension, so to speak, of these lines on one another; all depending on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions. In the foregoing experiments the dielectric was air; but they were afterwards varied by substituting a fluid, as oil of turpentine, and likewise a few solid dielectrics, namely, shell-lac, sulphur, carbonate and borate of lead, flint-glass, and spermaceti, and with these, corresponding results were obtained. These results, the author considers, cannot but be admitted as arguments against the received theory of induction, and in favour of that which he has put forth.

In the course of these experimental researches, some effects due to conduction, which had not been anticipated, and which were similar to the residual charge in the Leyden jar, had been obtained with such bodies as glass, lac, sulphur, &c. If the inductive apparatus, fitted with a hemispherical cup of shell-lac, after having remained charged for fifteen or twenty minutes, was suddenly and perfectly discharged, and then left to itself, it would gradually recover a very sensible charge; the electricity which thus returned from an apparently latent to a sensible state being always of the same kind as that given by the charge. This return charge is attributed to an actual penetration, by conduction, of the charge to some distance within the dielectric at each of its two surfaces, and several experiments are adduced in support of this view. With shell-lac and spermaceti the return charge was considerable; with glass and sulphur it was much less; but with air, no decided effect of the kind could be obtained. As this was an effect which might interfere with the results, in the method the author adopted for deciding the question of specific inductive capacity, and as time was requisite for this penetration of the charge, its influence on these results was guarded against by allowing, between the successive operations, as little time as possible for this peculiar action to arise.

The author thus states the question of specific inductive capacity which he had proposed to investigate:—Suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A, at equal distances, and uninsulated; A will then induce equally towards B and C. If in this position of the plates, some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same; or will the relation of C and B to A be altered by the difference of the dielectrics interposed between them?

The experiment of Coulomb, from which it appeared that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body, as the same body took in air, seemed to the author to be no proof of the truth of the assumption, that, under such variation of the circumstances as he had supposed, no change would occur. Entertaining these doubts as to the conclusions deducible from Coulomb's result, he had the apparatus previously described constructed, as being well adapted for this investigation. After rejecting glass, resin, wax, naphtha, oil of turpentine, and other substances, as unfit for the purpose in view, he chose shell-lac as the substance best calculated to serve as an experimental test of the question.

For the purpose of comparing the inductive capacities of shell-lac and air, a hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus, so as to nearly fill the lower half of the space between the two spheres; and their charges were divided in the manner already described; each apparatus being used in turn to receive the first charge, before its division with the other. As the two instruments were known to have equal inductive powers when air was contained in both, any deficiencies resulting from the introduction of the shell-lac would show a peculiar action in it, and, if unequivocally referable to a specific inductive influence, would establish the point in question.

The air apparatus being charged, and its disposable charge being 290°, this charge was divided between the two. After the division the charge in the lac apparatus was 113°, and in the air apparatus 114°. From this it appears, that whilst by the division the induction through the air lost 176°, that through lac gained only 113°. Assuming that this difference depends entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then the capacity for electric induction would be inversely as the respective loss and gain; and assuming the capacity of the air apparatus as unity, that of the shell-lac apparatus would be $\frac{176}{113}$ or 1·55.

When the shell-lac apparatus was first charged, and then the charge divided with the air apparatus, it appeared that the lac apparatus, in communicating a charge of 118°, only lost a charge of 86°. This result gives 1·37 as the capacity of the lac apparatus.

Both these results, the author considers, require a correction; the former being in excess, the latter in defect. Applying this correction, they become 1·50 and 1·47. From a mean of these and several similar experiments, it is inferred that the inductive capacity of the apparatus having the hemisphere of lac is to that with air as 1·50 to 1.

As the lac only occupied one half of the apparatus containing it, the other half being filled with air, it would follow from the foregoing result, that the inductive capacity of shell-lac is to that of air as 2 to 1.

From all these experiments and from the constancy of their results the author deems the conclusion irresistible, that shell-lac does exhibit a case of *specific inductive capacity*.

Similar experiments with flint-glass gave its capacity 1·76 times that of air. Using in like manner a hemisphere of sulphur, it appeared that the inductive capacity of that substance was rather above 2·24 times that of air, and the author considers this result with sulphur as one of the most unexceptionable.

With liquids, as oil of turpentine and naphtha, although the results are not inconsistent with the belief, that these liquids have a greater specific inductive capacity than air, yet the author does not consider the proofs as perfectly conclusive.

A most interesting class of substances, in relation to specific inductive capacity, the gases or aeriform bodies, next came under the author's review.

With atmospheric air, and likewise with pure oxygen, change of density was found to occasion no change in the inductive capacity. Nor was any change produced, either by an increase of temperature or by a variation in the hygrometric state.

The details are then given of a very elaborate series of experiments with atmospheric air, oxygen, hydrogen, nitrogen, muriatic acid, carbonic acid, sulphurous acid, sulphuretted hydrogen, and other gases, undertaken with the view of comparing them one with another under a great variety of modifications. Notwithstanding the striking contrasts of all kinds which these gases present, of property, of density, whether simple or compound, anious or cations, of high or low pressure, hot or cold, not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold among bodies having the gaseous form.

In conclusion, the author remarks, that induction appears to be essentially an action of contiguous particles, through the intermediation of which the electric force originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind and exactly equal in amount, but opposite in its direction and tendencies. Induction requires no sensible thickness in the conductors which may be used to limit its extent, for an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states, as long as the induction continues. But with regard to dielectrics, or insulating media, the results are very different; for their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever be their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of specific inductive capacities.

The author also refers to a transverse force with which the direct inductive force is accompanied. The experimental proof of the existence of such a force, in all cases of induction, is, from its bearing on the phænomena of electro-magnetism and magneto-electricity, of the highest importance; and we cannot but look forward with the greatest interest to the promised communication in which these and other phænomena relating to this subject will be reviewed.

January 18, 1838.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

Bryan Donkin, Esq,; Sir John Jacob Hansler; the Rev. George Henry Sacheverell Johnson, M.A.; and George Richardson Porter, Esq., were severally elected Fellows of the Society.

"On the Variation of a Triple Integral." By Richard Abbott, Esq. F.R.A.S. Communicated by Benjamin Gompertz, Esq., F.R.S.

In the calculus of variations, the discovery of which has immortalized the name of Lagrange, that illustrious mathematician, by differentiating the function with respect to a new variable which enters into it, reduced the general problem of indeterminate maxima and minima to the solution of an equation depending on the variation of the given integral, whether single or multiple, and whose differential coefficient contains any number of variables, or which even depends on other integrals. The author investigates, in the present memoir, the case in which the given function is a triple integral; its variation being composed of two distinct parts, namely, a triple integral and another part, the determination of which must be sought from the limits of the triple integral.

"Explanation of the Phænomena of Intermitting Springs." By

W. L. Wharton, Esq. Communicated by James F. W. Johnston, Esq., M.A., F.R.S. L. & Ed.

The author, considering the generally received explanation of intermitting springs, founded on the operation of a simple syphon, as being insufficient to account for the phænomena, inasmuch as the water which has risen above the lower side of the bend of the syphon will merely trickle down its longer leg, and be expended before it can fill the whole area of that part of the syphon, has proposed the following hypothesis for the solution of the difficulty. He conceives that the stream, while falling obliquely down the long leg of the syphon, is broken into drops, and carries along with it numerous air-bubbles, which, if the lower end of the tube have an abrupt bend upwards, will be impelled forwards, and escape at the open part; thus occasioning a rarefaction of the remaining air in the tube sufficient to ensure its full operation as a syphon. A model is described, which the author constructed for the purpose of illustrating and corroborating his views.

January 25, 1838.

FRANCIS BAILY, Esq., Vice-President and Treasurer,
in the Chair.

Neil Arnott, M.D.; the Rev. William Cureton, M.A.; and Charles Lock Eastlake, Esq., were severally elected Fellows of the Society.

A paper was in part read, entitled, "Fourth Letter on Voltaic Combinations." Addressed to Michael Faraday, Esq., D.C.L., F.R.S., by John Frederic Daniell, Esq., F.R.S.

February 1, 1838.

FRANCIS BAILY, Esq., Vice-President and Treasurer,
in the Chair.

The reading of a paper, entitled "Fourth Letter on Voltaic Combinations, with reference to the mutual relations of the generating and conducting surfaces;" addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c. By John Frederic Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London, was resumed and concluded.

In this communication the author describes a series of experiments, made for the purpose of determining the distribution of the voltaic force from its source in the generating metal, as indicated by the deposition of reduced copper in the constant battery; and, considering that the voltaic combination most perfect in theory would be one formed by a solid sphere, or point, of the generating metal, surrounded by a hollow sphere of the conducting metal, with an intervening liquid electrolyte, he constructed an apparatus making as near an approximation as possible to these conditions. It consisted of two hollow brass hemispheres, applied to each other by exterior flanges, and rendered water-tight by an intervening collar of leather. In the centre of the hollow sphere thus formed, a ball of amalgamated zinc was suspended by a well-varnished copper wire, connected with one of the cups of a galvanometer, and was contained in a membranous bag holding the acid solution; the whole being introduced through a short tube in the top of the upper hemisphere, and the remaining space being filled with a saturated solution of sulphate of copper. The galvanic circuit was completed by wires establishing connexions between either hemisphere and the other cup of the galvanometer. For measuring the forces developed, sometimes the ordinary magnetic, but in the greater number of instances the calorific galvanometer of De la Rive was employed; the indications given by these instruments were noted, on the completion of the circuit, in various ways; and the deposition of copper in the hemispheres was examined after the apparatus had been in action for a certain number of hours.

The following are the conclusions which the author deduced from a series of experiments thus conducted:

1st. The force emanating from the active zinc centre diffuses itself over every part of the upper hemisphere, from which there is a good conducting passage for its circulation.

2nd. The same amount of force is maintained by either hemisphere indifferently; but when both conducting hemispheres are in metallic communication there is no increase of force.

3rd. Although the force is not increased, it spreads itself equally over the whole sphere.

4th. When one hemisphere is connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere is connected by a long wire, through the galvanometer, with the same centre, the equal diffusion of the force over the whole sphere is maintained.

5th. There is no greater accumulation of precipitated copper about the point with which the conducting wires are brought into contact, and towards which the force diffused over the whole sphere must converge, than at any other point: proving that the force must diverge from the centre equally through the electrolyte, and can only have drawn towards the conducting wires in the conducting sphere itself. Other experiments showed that the force is but slightly increased by a great increase of the generating surface.

The author's attention was next directed to ascertaining the nature of the law according to which the force emanates from the zinc centre to the surrounding conducting sphere. With this view, a variety of experiments were made with the zinc in different positions in the interior of the sphere; and from these it appeared that, whatever may be its position, the whole force is the same. From these results it is inferred, that the force emanating from the zinc ball diffuses itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces being in the inverse duplicate ratio of the distance.

Experiments of the same kind were likewise made with the previous combination inverted, that is, with a small copper ball in the interior of a large hollow sphere of zinc; and from these the author concludes that, in this case also, the law of radiation is maintained, although the force is reduced to one half of that obtained from the former combination.

In order to ascertain the effect of cutting off the lateral radiation from the zinc ball, it was placed in a glass tube, six inches long, within half an inch of the lower aperture, over which a piece of membrane was tied, and the tube plunged into the solution of copper contained in a brass hemisphere, so as to rest upon the bottom. The results obtained by this arrangement, as also those when the zinc ball was raised in the tube to the surface of the solution, showed that the action of the zinc ball had been propagated from the aperture of the glass tube, as from a centre, diverging from this in the solution.

The experiments next described appear to have an important bearing on a question of vital interest in the theory of electricity, which has been discussed by Mr. Faraday, in a paper recently read to this Society: viz., whether the forces emanating from a centre of electric action act, like other central forces, in straight lines; or whether they are propagated from particle to particle in the surrounding matter, and may, consequently, when obstacles interfere with their rectilinear propagation, exert their influence in curved lines. An elliptical plate of copper, one side of which was covered with lac varnish, was placed in an earthen pan, with the varnished side upwards, and covered to the depth of a few inches with the acid solution of copper. The zinc ball, placed in the tube half an inch from the diaphragm, was plunged just below the surface of the solution, and the circuit being completed, the galvanometer indicated an action nearly equal to that which had been previously observed when both sides of the copper had been exposed. The under side of the copper presented the appearance of a border of precipitated compact pink copper, varying from 1½ to ⅞ of an inch in width, and the remainder was covered with precipitated copper of a darker red colour, into which the border gradually passed; and similar results were obtained with a circular disc of copper, having one side varnished. It hence appears, that the under surface, which, by itself, is capable of sustaining from the ball in the centre of the solution an action nearly as great as the upper surface, when combined with the latter adds no more than about one-eighth part of its efficiency; and whereas, with the upper surface, the action varies in some inverse ratio of the distance of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases: and this point is doubtless dependent on the angle at which the force which radiates from the ball meets the edge of the plate. The author having been led to the conclusion, that the force developed by voltaic combinations is subject to the law of radiant forces, had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; but having perused Mr. Faraday's "Eleventh series of experimental researches in

Electricity," all his own results appeared to fall in naturally with the general views therein explained. He considers, that the direction of the force through an electrolyte may be expressed in the very words employed in that paper to describe that of the direct inductive force in statical electricity, simply substituting the term *Electrolyte* for *Dielectric*, and the term *Current* for *Induction*.

Experiments are further described, in which the effects of various combinations of different generating and conducting surfaces, placed at different distances apart, were measured by the calorific galvanometer, from which the following conclusions are drawn:

1st. That the energy of the force is about sextupled by the absorption of the hydrogen at the conducting surface; except in the case of equal plates, when it is more than quadrupled.

2nd. That the effect of distance is much more decided in the instances where the amount of the circulating force is greater, than in the contrary cases.

3rd. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper, is, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc, exposing a surface of 3·14 square inches, placed over the centre of a plate of copper, exposing on its two sides a surface of 28 square inches, sustains an action of nearly the same amount as a plate of zinc, of the same dimensions as the copper, placed at the same distance.

In conclusion, the author remarks, that the principal circumstance which limits the power of an active point within a conducting sphere, in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may virtually be considered the same wherever the included point may be placed, but increases with the diameter of the sphere. In an insulated hemisphere, however, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte, and consequently the force increases. In this respect, the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which, as the point approaches, the force increases.

February 8, 1838.

STEPHEN PETER RIGAUD, Esq., Vice-President, in the Chair.

George Lowe, Esq., who, at the Anniversary of 1836, had ceased to be a Fellow, from the non-payment of his annual contributions, was, at this meeting, re-admitted by ballot into the Society, agreeably to the provision of the Statutes.

James Bateman, Esq.; Joseph Glynn, Esq.; William Hallows Miller, Esq., M.A.; the Rev. Joseph Bancroft Reade, M.A.; Robert Bentley Todd, M.D.; and Alexander Tweedie, M.D., were elected Fellows.

A paper was read, entitled, " Researches towards establishing a Theory of the Dispersion of Light", No. IV. By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

In his former communications to the Royal Society the author had instituted a comparison of the results of observation and of theory with regard to the dispersion of light, in the instances of the respective indices for the standard rays in fifteen different cases of transparent media; and had found a sufficiently close agreement in the cases which gave the lower numbers; but there yet appeared to be an increasing discrepancy as an advance was made towards the higher. The theoretical formula employed in this investigation was one derived from the undulatory hypothesis, by a process involving some limitations, which rendered it only approximative. By pursuing the calculations to a greater degree of developement, or by adopting methods of a more precise character, such as those of M. Cauchy and of Mr. Kelland, the author was led to hope that a more close coincidence might be obtained. The formulæ of M. Cauchy, however, involved calculations of so elaborate and overwhelming a character, that he was induced to make trial of the method of Mr. Kelland, applying it, in the first instance, to the case of the most highly dispersive substance, namely, oil of Cassia, in which the greatest discrepancy had before appeared.

The object of the present communication is to state the results obtained, together with the necessary data employed in the calculations; and also to elucidate the general method, so as to render it more easily applicable to other cases which may arise in the further prosecution of the determination of specific indices. For this purpose a general statement is given of Mr. Kelland's method, in whose formulæ, it is easy, knowing the value of the wave-length in air, and taking the indices as given by observation for that particular medium, to introduce the values of the wave-length in the medium. Two of the constants are then determined for that medium; and by the aid of these, combined with the indices given by observation, a value of the third constant is deduced for each ray: and the verification of the theory will result from the equality of the respective values of this latter constant thus obtained.

The author then gives tables exhibiting the comparison of observed refractive indices with the results of Mr. Kelland's theory; first, in the case of sulphuret of carbon, at a temperature of 12° (centigrade); next, of the same substance at 22°; and lastly, of oil of Cassia: from which it appears, that the accordance between the results of observation and of theory is sufficiently within the limits of the errors in the experimental data to satisfy all reasonable expectation.

A paper was also in part read, entitled, " Experimental Researches in Electricity." Twelfth Series. By Michael Faraday, Esq., D.C.L., F.R.S., &c.

A letter was read from Dr. Marshall Hall, in reply to a note contained in the paper of Mr. Newport, published in the last volume of the Philosophical Transactions.